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Renaissance or Killer Mutation? A Response to Holburn

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Holburn (1997) has done behavior analysts a useful service by describing person-centered approaches (PCAs) and showing how a behavior analyst in a residential setting might fit into them. His intent is to recast the role of the behavior analyst in such settings so that behavior analysts are seen less as "behavior police" (not his term, but he quotes it) and more as positive contributors to the behavior change of "focus persons" in residential settings. His intent is positive; his tone is positive; he means well. Indeed, everyone in the PCA movement means well. We should feel good about PCAs, shouldn't we?

Holburn's focus is the residential environment, and he all too correctly describes some of the conditions that can exist therein—boredom, implicit aversive control, overregulation, regimentation, underfunding, undertraining—and our seeming inability as a society to come to grips with them. Ostensibly, PCAs will resolve these problems. This may be implicit in their adoption. But, will they?

The movement toward PCAs has consequences for residential behavior analysis specifically and applied behavior analysis in general. If PCAs are adopted wholesale, residential behavior analysts may well become the caretakers of their settings—taking roll, schedule keeping, advising, participating in many meetings—and generally being thought of as nice people, but otherwise not effective. At the risk of being branded nonhumanistic, this response intends to suggest that applied behavior analysis is on a course of development by its association with the PCA movement, without much in the way of examination of the consequences of that development.

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Some Difficulties with PCAs

Here is a brief list of the difficulties with PCAs as seen by one behavior analyst:

1. PCAs are the newest faux fixe in the never-ending list of such fixes (e.g., assertive discipline, gentle teaching, total inclusion, facilitated communication, patterning, try another way) promulgated by well-meaning people to satisfy the universal urge to do something effective with very difficult problem behaviors and the people who exhibit them. PCAs are also supported by other, less well-meaning people (e.g., some state bureaucrats) as another way to dodge the inevitable accountability problems that surface when techniques have existed long enough for someone to ask how effective they are.

2. Faux fixes are palliatives, and are responses to the extant cultural milieu that today is exemplified by political correctness. Indeed, PCAs may be thought of as politically correct approaches. Consider a few of the ways in which PCAs are politically correct: (a) Individual responsibility for a "focus person" is dropped in favor of a team's responsibility, thereby diminishing individual responsibility. (b) Catchy phrases are adopted (e.g., get a life, positive behavioral support, wraparound services, focus person, etc.). (c) Technical vocabulary is downplayed. (d) Postmodernism and constructivism are invoked (Sailor, 1996). (e) The im-

portance of contingency is moot. This last point may be critical to behavior analysis, with its very central focus on contingency. My computer search of the literature on person-centered approaches found that the majority of abstracts were related to Rogerian psychotherapy, with its central notion of the absence of contingency. Could there be an implicit relationship between the PCAs behavior analysts are involved in and Rogerian psychotherapy? It is one thing to examine the effects of noncontingent reinforcers on self-injury, as Iwata and his colleagues have adroitly been doing (e.g., Fischer, Iwata, & Mazaleski, 1997; Marcus & Vollmer, 1996; Mazaleski, Iwata, Rodgers, & Vollmer, 1994; Vollmer, Iwata, Zarcone, Smith, & Mazaleski, 1993), or the effects of contingent and noncontingent context manipulations, as Carr and his associates have been doing (e.g., Taylor & Carr, 1992a, 1992b), and quite another thing to begin with the premise that contingency is irrelevant. Yet, it is not an untenable inference that PCAs do not find the contingency to be of primary importance.

3. Faux fixes are constructed in such a way as to preclude or at least to ignore analysis of their putative effectiveness, and, as such, they usually are, if not openly antiscientific, ignorant of scientific rigor (cf. Green, 1995). Traditional psychoanalysis, the classic faux fixe, shunned analysis for years.

Consider that when we finally pose the big question—Does person-centered planning work?—we get an answer from Holburn that states that we shouldn't have asked precisely that question. Instead we are given to believe that whether it works might be difficult to measure ("not well represented on a graph or in a table," Holburn, 1997, p. 80), incredibly because the changes it may produce are "multiple and profound" (Holburn, 1997, p. 80). Of course, if the changes one produces are truly multiple and profound, graphs and tables may be irrelevant to seeing them, but such measures and their underpinnings are part of the essence of an applied science of behavior. Perhaps Holburn has simply made an adjustment to his own circumstances, because he informs us that he is contrasting 20 people on a PCA with 20 who are not (p. 80) and is conducting a case study to boot (p. 81). So although he adopts a new modus operandi—a seemingly nonscientific one—he has not totally given up on the scientific method and an applied science of behavior. Perhaps saying one thing and doing another are necessary to get along?

4. Faux fixes promise much, and because of that people feel good and are positively persuaded that they are doing the right thing by adopting them (Jacobson, Mulick, & Schwartz, 1995). But essentially no one will ever know how effective the fix is because of the absence of functional analysis; such absence is the very condition that precludes it being shown that the fix has little or no effect. Feeling good about the faux fixe is good enough in the current culture.

5. Faux fixes fly in the face of extant knowledge of the difficulty of producing meaningful behavior change in individuals and in institutions (residential or other). They purport to accomplish the fantastic (e.g., the claims for facilitated communication) with ease.

6. Attempts to examine faux fixes are met with scorn because of epistemological differences between those who promulgate the fix and those who would examine it (Jacobson et al., 1995).

7. Usually, but not always (e.g., the sexual abuse charges that have resulted from facilitated communication), nothing much happens with the adoption of a faux fixe, except that a few consultants, using the new jargon created to describe the fix, make or raise money. Meanwhile, time when effective procedures could have been brought to bear on the system is wasted, and the focus person stays in the bureaucratic maze created by the fix, until the cycle turns to the next faux fixe. As such, it

has been argued, faux fixes "are not benign [because] they supplant use of proven and reliable methods when these methods do not also appear to produce dramatic breakthroughs" (Jacobson et al., p. 762). It is also reasonable to add that by default they take resources away from proven practices. The cyclical nature of faux fixes suggests that they create moving targets. That is, as soon as one is spotted and critiqued, another emerges.

Applied Behavior Analysis and Person-Centered Approaches Contrasted

Let us examine specifically the fit between PCAs and applied behavior analysis in terms of the standard three criteria, that is, the well-accepted meanings of *applied, behavior,* and *analysis* (Baer, Wolf, & Risley, 1968). *Applied* means socially significant behavior change. There is no question that, if PCAs produce socially significant behavior change, they will meet the socially significant criterion. However, is one of the tenets of PCAs socially significant behavior change? Or is it just improving life? As Holburn points out, life may be improved, and behavior may not change thereafter. As a not unimportant aside, what happens then? Is life functionally unimproved if behavior does not change, even though it is operationally improved? And how will we know (see below)? That is, how will we know that life has improved for a person with severe developmental disabilities if their behaviors have not changed?

This reasoning carries us to the second characteristic of applied behavior analysis: the focus on measurable behavior. Holburn incorrectly labels us all methodological behaviorists, which will cause the radical behaviorists among us, including myself, some stress. He is correct to paint the history of applied behavior analysis with a focus on observable behavior, but he is incorrect thereafter to imply that we are all methodological behaviorists because of this focus (post hoc, ergo propter hoc). Rather, the challenges that were dealt to behavior analysts in the 1960s and 1970s provided the context for the focus on extremely difficult behaviors, most of which were eminently overt and did not involve private events directly. If a particular technique (borrowed initially from the laboratory) worked, why entertain thoughts about private events? Behavior analysts simply proceeded to the next of a never-ending succession of overt problem behaviors to be changed, and philosophy about private events stayed on the back burner.

In any case, the focus of applied behavior analysis is clearly on behavior, whereas PCAs appear to minimize this focus in favor of operations that have a cozy feel to them. The outcomes Holburn lists as a part of his own study of the effectiveness of a PCA are illustrative and representative. He states that, "Problem behavior is not a . . . focus" (p. 81), although it is apparently measured. Obviously, this constitutes a major difference between PCAs and applied behavior analysis. The emphasis of PCAs is much more on operations and less so on their resultant effects. Holburn's list contains a measure of the fidelity of the planning process, a measure of internal validity, which is commendable. It also contains measures of quality of life; types, frequency, and integration of activities; and categorical life changes. These all appear to be procedural, and all are measures of internal validity. Nowhere is it stated that these are related eventually to behavior change, that is, that the procedures have external validity. The same appears to be true in the literature (e.g., Koegel, Koegel, & Dunlap, 1996) in which authors note anecdotally and only in passing that challenging behaviors were greatly reduced, or that communication was enhanced, or that the focus person had not been readmitted to an institution. Although these are all lovely outcomes, if, indeed, they are valid, they are not necessarily measures of behav-

ior (see Holburn's list above), or if the outcomes do refer to behavior, the reference is anecdotal. PCAs appear to fail the criteria for behavior in applied behavior analysis.

Risley's PCA (Risley, 1996) is more theoretically grounded. At the most general level are quality-of-life and developmental issues. At a more specific level are the contingencies and management issues of everyday living. At the most specific level Risley locates behavior analysis, which, he asserts, only a few psychologists are capable of conducting. (Does he mean non-behavior-analytic psychologists, or should we be offended?) This systems approach leads him to conduct behavior analysis, when necessary, only after programming occurs at more molar levels (e.g., team building, life arrangement). In Risley's approach, behavior analysis has a clear, but ostensibly minor, role to play. Here, at least, the baby and the bathwater are explicitly joined.

Finally, applied behavior analysis is characterized by analysis (Baer et al., 1968), nicely documented by Holburn in the historical part of his paper. *Analysis,* of course, means the inclusion of research designs and a strong focus on behaving scientifically toward the subject matter. Obviously, this includes operationally defined independent and dependent variables, the subsequent functional relation of the two, and the science that goes with them (cf. Johnston & Pennypacker, 1993). PCAs seem to include a hazy operationism but passively ignore the necessities of functionally relating independent and dependent variables. Indeed, Holburn's narrative suggests a cant toward qualitative research—to wit, storytelling, another focus that is politically correct and potentially antiscientific. However, on this point there is no wholesale objection. Effective public speakers tell good stories, and this includes behavior analysts. Indeed, who could forget the stories told in the Lovaas films, which portrayed the startling before, during, and after images that represented behavior change in autistic children, produced by applied behavior analysts? They are, simply, fine effective communications. But, they are not the primary data of our field. (Exceptionally, the Lovaas films were chock full of primary data.) Perhaps it is not too early to worry that stories may be the primary "data" of PCAs?

Conversion? Unlikely

Holburn asserts that it will not be easy to convert to person-centered approaches. On this point, he is absolutely correct! However, he assumes that we should want to convert because through such a conversion we may better help to change behavior in important ways. On this, he is absolutely incorrect, for to do so, it appears that one must relinquish the fiber, bone, and guts of behavior analysis. Moreover, the foregoing aside, why should one be interested in conversion when the very societal contingencies that have thwarted applied behavior analysis are present in regard to PCAs (i.e., absence of control over the larger institutional and societal contingencies)? Clearly, the simple awareness of these problems by those in the PCA movement is insufficient.

Can we hope that those behavior analysts who are already involved in PCAs participate because they have to? That this is a way for them to continue to shape others to behavior-analytic principles? That this is a way for them to continue to get federal and state funding? That this is a way for them to continue to exist? Unfortunately, to be a part of a team, the very business of which demands a refutation of much of what behavior analysis holds dear, means that one experiences the social contingencies therein and is changed by them. To behave as an unrevised behavior analyst on such a team will surely risk censure.

Summary

Holburn (1997) argues that applied behavior analysis in its original form has become increasingly irrelevant. His

position is that behavior analysts in residential settings are so severely restricted now that they cannot be effective. So, it seems, to still be involved in the process of behavior change, albeit at some reduced level in PCAs, most residential behavior analysts should feel a sense of gratification at their inclusion in a team that will conduct person-centered planning.

It is easy to refute the broad statement that applied behavior analysis is becoming increasingly irrelevant. The Association for Behavior Analysis continues to grow, the number of behavioral journals continues to increase, and there is increasing demand for behavior analysts in the treatment of autism, in animal training, and in business and industry, as examples.

But, is Holburn correct with respect to residential behavior analysis? In the residential situation has the behavior analyst been restricted to the point at which he or she can no longer be effective? I am not in a position to argue that point broadly, but my own recent experience (8 years of consultation at a state ICF-MR) suggests otherwise (e.g., Osborne et al., 1992, 1994; Osborne, Peine, Darvish, Blakelock, & Jenson, 1995; Peine et al., 1995; Peine, Darvish, Blakelock, Osborne, & Jenson, 1998; Peine, Liu, Blakelock, Jenson, & Osborne, 1991). On the other hand, that consulting relationship has waned, and the institution is currently fiddling with a PCA. In so doing it has reduced the size of its psychological staff (primarily applied behavior analytic) and has increased the caseloads of those remaining. Is this a common outcome when an institution moves toward person-centered planning? It suggests the reduced relevance of behavior analysis in the scheme of things.

Assume for a moment that Holburn is correct about residential behavior analysis. Is participating in a PCA as the way out of that dilemma the best we can do? What if there is no renaissance in so doing? What if there is a terminal mutation? Do we need dialogue? Or is it already too late?

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